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Name of Star.	Newcomb (1872).		Gyldén (1875).		Auwers (1875).	
	I.	II.	I.	II.	I.	II.
	s	s	s	s	s	s
α Ophiuchi	·036	·007	·026	·010	·042	·016
α Lyræ	·053	·024	·011	+·005	·054	·028
γ Aquilæ	·038	·009	·010	·006	·027	·001
α Aquilæ	·036	·007	·018	−·002	·027	·001
β Aquilæ	·025	+·004	−·010	+·026	·013	+·013
α^2 Capricorni	·018	·011	−·005	·011		
α Aquarrii	·026	·003	·006	·010	·028	−·002
α Piscis Aust.	·027	·002				
α Pegasi	−·023	+·006	−·001	+·015	−·015	+·011

The numbers under the columns headed I are the actual differences between the Catalogue places, those under the columns headed II are the differences when a constant correction of $-^{s}{\cdot}029$ is applied to Newcomb, $-^{s}{\cdot}016$ to Gyldén, and $-^{s}{\cdot}026$ to Auwers.

1879, *Jan.* 8.

On the Effects of "Personality" on the Tabular Errors of the Moon. By E. Dunkin, F.R.S.

In Mr. Neison's interesting Paper on the effects of personality on the Greenwich tabular errors of the Moon (*Monthly Notices*, vol. xl. pp. 75–80) there are one or two points having reference to some expressions and deductions of mine contained in a Paper inserted in vol. xxix. of the *Monthly Notices*, on which I think I ought to make a few remarks, principally, however, in the way of explanation. The subject has for many years been one of considerable interest to me, since, in fact, the year 1848, when the remarkable personal equation in observing the limbs of the Moon between Mr. H. Breen and myself was first noticed.

The first point to which I wish to call attention is the paragraph in Mr. Neison's Paper in which he states that "it appeared that Mr. Dunkin had based his investigation on the assumption that 'the mean tabular errors are assumed to be constant throughout the mean lunation.' It is now known that this assumption is not a permissible one, for the tabular errors are not constant throughout the mean lunation, but that, owing to imperfections in the theory, they are systematically different before and after full Moon." Through a fault of my own in the construction of the sentence, Mr. Neison has been led unfortunately to misunderstand my meaning in the above quotation, for the information I intended to convey to the reader was, "provided the (relative) mean tabular errors are assumed to be constant

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throughout the mean lunation." This remark, I contend, is sufficiently accurate for the purpose I had in view, knowing that the four observers had an equal and regular share in the observations. I was not concerned in determining the varying absolute amounts of tabular errors in the course of the year; all that I assumed was that, in the mean yearly groups of the different observers, the relative tabular error might be considered to be constant; and I had specially mentioned that, to ascertain whether the mean relative tabular errors represented the same point in the Moon's orbit, I had arranged all the errors for each observer according to the age of the Moon, when it was found that the mean age agreed sensibly for all, for observations both of the first and second limbs. It appears to me that the general accordance of the results exhibited in the tables given in my former Paper is a sufficient proof that they can only be slightly affected by any relative difference of tabular error; and probably, if I had to go over the work again, I should make no material modification which could sensibly influence the results.

I do not clearly understand the process by which Mr. Neison obtains the numbers inserted in the table on page 79. It appears to me that we are not at all certain that personal equations are so constant as to assume that no change has taken place in that of any person between 1863 and 1876; indeed there is some evidence that changes have actually occurred in personal equations when derived from the observations of the transits of stars. For this reason, I rather doubt the advisability of comparing, by an indirect method, the observations made by me in 1863–69 with those made by Mr. Thackeray in 1875–76, especially as the observations of the latter were far too few in these years to give a reliable determination. As an example of a progressive change of personal equation, I have only to refer to the case of Mr. Rogerson, in whom it varied in the eight years from 1846 to 1853 from $0^s{\cdot}31$ to $0^s{\cdot}67$, by a nearly equal increment each year. We ought therefore to be quite certain of our data before coming to a conclusion, especially in regard to such small quantities as we are obliged to deal with in investigations of this nature.

Now I must ask the question whether Mr. Neison has had reliable data before him with regard to the more recent observers, sufficient to prove that "the change of observers has therefore introduced a considerable increase in the mean tabular error of the Moon which has no real existence." (P. 79.) I think not. I have been led to this opinion from an examination of the tabular errors of longitude of the Moon determined in 1877 and 1878 when we have had a fair number of observations made with the Transit-Circle by Mr. Downing and Mr. Thackeray to deal with. I have grouped together the errors under the names of Mr. Lynn (who observed only in 1877 and in the early months of 1878), Mr. Criswick, Mr. Downing, and Mr. Thackeray; the means were then taken and referred to Mr.

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Criswick's mean as the standard. The following is the result:—

	1877. ″	1878. ″	Mean 1877–78. ″
Lynn —Criswick =	+0·54	...	+0·54
Downing — „ =	−1·02	−0·27	−0·72
Thackeray— „ =	−1·02	+0·95	−0·10

These numbers seem to show that we must look to some other cause than that assigned by Mr. Neison to explain any increase in the mean tabular error of the Moon. The figures point towards a decrease in the error rather than an increase; but they also prove that it is unsafe to base any particular theory on results liable to such fluctuations as those exhibited in this table.

Kidbrooke, Blackheath,
1880, *January* 8.

Note on the Remeasurement of a Lunar Photograph, in reply to Mr. Neison's Criticism. By Prof. C. Pritchard.

In the *Monthly Notices* of November last, Mr. Neison did me the honour of criticising a paper of mine laid before the Astronomical Society in June of the same year. In that paper I presented to the Society what seemed to me to be a question of considerable novelty and interest, viz. the measurement of the Moon's mean semi-diameter from a series of photographs taken by Mr. Jenkins with Mr. De La Rue's Reflector, the admirable defining power of which is generally acknowledged. I explained that the computation ef the Moon's diameter was not the ultimate object for which the photographs were originally taken, but that, if I found a considerable number of them, taken at random out of many hundreds, gave accordant results, this circumstance would give me confidence in applying the fine series of photographs in the possession of the University of Oxford to the determination of the lunar physical libration; the latter investigation, it may be observed, involving an enormous expenditure of labour, and not to be undertaken without adequate grounds of confidence. The results of the computation of this diameter exhibited an amount of accordance which not only satisfied, but surprised me, when I reflected on the discordances arising from other known methods of observing. These results of very laborious measurement and computation I laid before the Society, and I thought them creditable both to the photography and to the computation.

Mr. Neison, however, in his criticism thinks it right to say: "Until we know the method by which these measures have been reduced, it is impossible to say how far the above result indicates